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ABSTRACT

Four papers on perceptual processes, by (1) W. Ahlbrand; (2) E. Elenbogen, S. Sperry and G. A. Thompson; (3) B. Randhawa; and (4) T. Sticht and D. Glasnapp, which were presented at the 55th annual meeting of the American Educational Research Association, are critiqued here. Emphasis is upon the relevance of the papers to issues in perceptual development. It is suggested that word perception may be a direct process rather than the resultant of phoneme or letter discriminations. The importance of knowledge about perceptual development for education is noted. (Author/AMM)

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A Critique of Papers on Verbal and Non-Verbal Influence on Perception

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Before commenting upon the research reports we have just heard, I would like to spend a few minutes describing a view of perception which is both implicit in some of those reports and important for education. It seems to me that a good number of introductory psychology texts convey the idea that perception is a relatively low level psychological process whose main function is to serve up pictures, sounds, and other sensations to higher processes. The papers we just heard do not seem to subscribe to this subordinate view of perception. They suggest to me that perception has to do with words, patterns, and even selves; what Eleanor Gibson (1969) calls "higher-order stimulus variables."

It seems to me that if we admit to such complex goings-on within the domain of perception, a somewhat new set of questions become important in educational research. For instance, we would become interested in the kinds of stimulus information children detect at different developmental levels. And we would want to know how the psychological processes responsible for perception change with age. In short, the nature of perceptual development becomes a pertinent area of inquiry for the researcher in education. With

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a good description of children's information processing at hand, the curriculum planner would be on firmer ground than he is at present.

The paper by Elenbogen, Sperry and Thompson (1971) touches on an interesting problem in perceptual development. You will recall that these researchers found no social class difference when nonsense phonemes were substituted for words on the Wepman test. The conclusion of Dr. Elenbogen and here colleagues is that class differences on the Wepman are probably due to familiarity with the test words and certainly not due to differential auditory discrimination ability.

Actually, the Elenbogen et al research found no class difference in phoneme discrimination. I am not sure it is accurate to equate phoneme discrimination with auditory discrimination. Word discrimination too, seems a legitimate aspect of auditory discrimination. In other words, perhaps we should conceptualize different levels of auditory discrimination each of which is defined in terms of the stimulus class discriminated.

It might be objected at this point that since spoken words are simply meaningful combinations of phonemes, it is phoneme rather than word discrimination which reflects auditory discrimination ability. Word discrimination seems to involve semantic processes which are more complex than mere perception. While it seems

incontestable that phoneme discrimination is a necessary condition for word discrimination, the perceptual mechanism by which words are detected may be different from that responsible for phoneme perception, but still a perceptual process which is a legitimate aspect of auditory discrimination.

Let me try to clear up some of the confusion I am doubtlessly causing. The research which gives rise to the points I have raised was performed recently at the University of Michigan (Reicher, 1968, Wheeler, 1970). Letters and words were presented visually rather than phonemes and words presented auditorally, so the research was somewhat different from that of Elenbogen et al. Its pertinence is that it was addressed at the problem of whether word perception is a product of perceiving the letters which constitute words.

Very briefly, Ss were presented either a word or a single letter for an extremely short duration. Then, two test letters appeared. Regardless whether S had been presented a word or a single letter he was to select that test letter which had been presented. Since words are composed of letters and Ss did not know which position in the word the target letter would occupy, we would expect better performance in the single letter condition than in the word condition. But just the opposite was found. The correct letter was more likely to be chosen if it had appeared in a word

than if it had appeared alone. Incidentally, words were chosen so that both test letters produced familiar words. For instance, if HEAD were the stimulus word, the first position test letters would be H and R (READ). The effect has been examined in, and has survived, a thorough series of experiments aimed at eliminating artifactual bases for it. The present conclusion is that word perception seems unaccountable in terms of letter perception.

I know of no research which so persuasively demonstrates that word perception involves a process different from that responsible for phoneme perception. But the Michigan research makes it at least reasonable to entertain such a possibility. In which case, returning to Dr. Elenbogen's report, the Wepman test may assess the basic auditory discrimination of words; which is certainly an important skill for education. Since we now know that phoneme discrimination does not differ between lower- and middle-class children, it would be interesting to learn whether or not word discrimination is related to class. The pertinent experiment should, of course, select test words for the Wepman on the basis of their familiarity to the children in each class.

The experiments reported by Sticht and Glasnapp (1971) are also concerned with the perception of words, in this case with the relationship between intelligence and word perception. I have several questions and comments about these experiments which

may hopefully instigate a discussion of them.

First, the absence of an Aptitude x Speech Rate interaction in the first experiment suggests that compression did not have a greater effect on the low MA group than on the high MA group. Yet the study is interpreted as finding that the low MA group was more affected by compression than was the high MA group. Perhaps Dr. Sticht can resolve this apparent discrepancy.

Second, I wonder if a trend test was carried out for the low MA group across the levels of speech rate. We are told that the result of such a test on the data of the high MA group was detection of a quadratic component. My question derives from the noticeable similarity in the shapes of the curves for the two MA groups.

Two aspects of Sticht and Glasnapp's Experiment 2 seem to warrant some elaboration. The high and low MA groups apparently performed equivalently in the baseline condition of Experiment 2. But in Experiment 1 there seem to have been large baseline differences between aptitude groups. Was the difference in stimulus materials between the two experiments responsible for these baseline differences? Second, the major hypotheses of Experiment 2, the high MA Ss would learn both chunkable and non-chunkable words under normal speech rate while low MA Ss would learn primarily the

chunkable words, was not supported. In addition, the table of means indicates that under compressed conditions low MA Ss showed a greater increase from baseline level for non-chunkable than for chunkable words. These two findings seem quite damaging to the hypothesis that aptitude differences on the task are due to differential ability to capitalize upon non-chunkable words. Do the authors intend to pursue this hypothesis or are other sources of the aptitude difference being considered?

I regret that I have little to offer on Dr. Randhawa's (1971) study. I received only an abstract of it and was unable to criticize the research meaningfully on that basis. I would be interested in hearing more about Dr. Randhawa's developmental model, however. In addition, I admit to an uneasiness about the "bits" approach to developmental information-processing. My pessimism stems from the meagre understanding of development which has accrued from this task in the past. Perhaps this will change. However, it would seem a more effective strategy to try to define uncertainty for each developmental level individually rather than independently of developmental level. Unfortunately, I can offer no technique for doing so.

One last remark on Dr. Randhawa's research: the output conditions may be amenable to developmental analyses. Maccoby and Bee

(1965) have an interesting paper on what they call the developmental lag between perceiving and performing.

Finally, the paper by Ahlbrand (1971) is concerned with perception in a different sense than that of the other three reports. I am not knowledgeable about sociometric tests and am therefore unsure of how the self-concept data was obtained. It seems that low participators gave an equal number of positive ratings to high and low participators. Is this the data base for inferring poorer self-concepts among low participators? Or was a more direct self-concept test included within the sociometric so that Ss rated themselves on a number of dimensions? I wonder if a better index of participation would be a combination of frequency and duration of speaking rather than frequency of speaking and time in class? Also, I would think that both the teachers' feelings about individual pupils and the degree to which discussion periods were "open" would strongly affect participation scores.

In closing, let me reiterate my convictions that research on perceptual development is fundamental to structuring meaningful education for children. These papers suggest that perception is not confined to low level processes. Words, patterns and selves are perceived. We need to know more about how these complicated perceptions develop if education is to be effective.

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Footnote

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